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" *To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

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THE LAWS OF HEREDITY.

The Laws of Heredity. By G. Archdall Reid. With a diagrammatic representation by Prof. H. H. Turner. Pp. xi+548. (London: Methuen and Co., Ltd., 1910.) Price 21*s.* net.

DR. ARCHDALL REID confesses that he is an "extreme Darwinian." It is interesting that he has reached this position from the study of the human species. He finds that this "vast field of research has been left practically untilled by students of heredity." He is not, properly speaking, a naturalist; in fact, he has rather a poor opinion of naturalistic work, and especially, I am sorry to say, of botanical. This is the more remarkable as Darwin himself loved "to exalt plants," and largely drew upon their study for his theory. The author is, however, a physician who, unlike most of his calling, is not satisfied with being empirical. He finds himself "able to watch, under conditions ensuring great accuracy, the tremendous and crucial experiments made by nature." With many of his results we are already familiar from his previous writings. They are beginning to obtain general acceptance; in proportion as they do so, they must profoundly change our mode of dealing with social problems of the utmost importance.

The object of the present work is apparently to set out the results of his investigations in a systematic form, and to show that they can be exhibited as deductions from widely accepted principles. The method has undoubtedly the advantage that it has enabled him to look at the whole subject from a new point of view, and to bring a very acute criticism to bear upon a good many questions on which opinion at the moment is much divided.

As Mill long ago pointed out, all science tends to become deductive, and biology cannot be excluded. But the progress which any particular science can make in this direction altogether depends on the certainty which attaches to the assumptions or propositions with which we start. And where the phenomena, as in the case of biology, are complicated and obscure, the difficulty must always arise as to whether the proposition we start from is really exhaustive of the fact. The validity of the conclusion cannot exceed that of the premises. Lord Kelvin's attempt to determine the age of the earth is an example. The conditions of the problem have proved to be insufficient, and I suppose no physicist would now refuse an evolutionist a blank cheque as to time.

Darwin himself linked together a number of separate inductions into a more comprehensive one from which he then argued deductively. Dr. Archdall Reid has continued the process, and in the first ten chapters of his book has attempted a synthesis of existing evolutionary theory. It is to be noted that when this is done the order of exposition is rarely that in which discovery was made. This is well known, for example, to be the case with the text-book treatment of the Newtonian theory. The process is, however, valuable, as it not merely brings to light a clear chain of causation, but by vigorously testing the strength of each link, often reveals unsuspected weakness, and may even suggest new discovery.

The author accepts and starts from Weismann's theory of the continuity of the germ-plasm. From this he makes the fundamental deduction that "individuals, for example, men, are nothing more than dwellings which the germ-plasm builds about its germinal descendants." Thence it follows "that the child inherits nothing from his parent." What it does inherit is nothing more than what was "inborn" in the germ-plasm from which it started. The germ-plasm, under the stimulus of nutrition, reproduces itself, and also produces the enveloping soma. But the latter also requires the stimulus of use ("injury" may be regarded as use with a minus sign), as well as that of nutrition: a limb will not reach full development unless used, and mental powers will remain dormant unless exercised. But the characters so developed are "rooted, as it were, in the germ-plasm." They flow from it: the question which has long divided biologists is whether modification of those characters produced by the stimulus of use can flow back and be transmitted to a succeeding generation. Darwin latterly apparently thought they could.

Herbert Spencer built upon their doing so his ethical system. "Most biologists reject the Lamarckian doctrine," on the ground that it is against the weight of evidence. That is my own position. But the author himself admits that there is some evidence in its favour, yet unhesitatingly also rejects it on deductive grounds. The argument is rather subtle; but it amounts to this: a character which develops under the stimulus of use cannot develop under the stimulus of nutriment alone. If it did so it would be "a miracle." But I am not sure that this is not an assumption. In a unicellular organism the soma and germ-plasm are identical, and as we rise in the scale of plants the separation of the germ-plasm is far from being as complete as it is in animals. In many plants, as in the well-known case of Begonia, a somatic cell will reproduce the whole individual, germ-plasm and all. I am not prepared to assert that the new germ-plasm is free from derived somatic influence. On the other hand, I know of no reason to think that it is not.

The Lamarckian doctrine being dismissed, natural selection is examined. Like Prof. Karl Pearson, Dr. Archdall Reid infers this immediately from "selective mortality" in mankind. He points out that this cannot be proved in the case of "wild plants and animals," but "presumably" it occurs. I doubt if disease is a dominant selective factor in nature, though no doubt it has been occasionally operative, and on a large scale. He puts the theory on too narrow a basis, and ignores the struggle for existence. What plants have to fight for is room to perfect their seeds and space for them to germinate.

This is not the only short cut to the root of the matter. "The plain fact that living beings are able to exist is a proof of adaptation." It does not appear to me to be self-evident, though Paley would probably have agreed. Anyhow, it is rather like trying to enter Darwinism by the back-door instead of toiling up the steps. I collect a somewhat better argument. Man is "manifestly a bundle of adaptations." "The growth of modern physiology implies merely an increased power of interpreting human traits in terms of their utilities." "Presumably adaptation is not less perfect in plants and lower animals than in man." Yet, as Rolleston used to tell us at Oxford, that sort of statement would not convict a poacher. Fortunately, evolutionists have a better case for the court.

Next we come to variation, which affords the material for natural selection to work upon, and some important conclusions are arrived at. Excluding any possible influence of the soma, and I agree, variation must be resident in the germ-plasm. "Reasoning by analogy," it is inferred that this is itself "established and maintained by natural selection." This involves the paradox that it preceded that which produced it. "Its origins are lost in obscurity." No doubt; but if I may try my own hand at deduction, I would suggest that primitive variation was a necessary consequence of molecular instability, and as I regard natural selection as a sort of physical principle like "least action" or gravitation, it would begin to operate at once.

The most fundamental point in the whole argument is the relation of the germ-plasm to the environment. Here two classes of facts have to be faced; first, the undoubted one, on which I have often insisted, that a few years' cultivation of a wild species breaks down its stability; and, secondly, such cases as the supposed degeneration of European dogs in India. I can only accept variation at present as an unresolved phenomenon. I have never contended that the environment could act as more than a stimulus to it, and I have no doubt that it does. Someone has used a better expression in saying that it pulls the trigger. To suppose that it has any directive action lands one at once in Lamarckism. The degeneration question is much more serious. To attempt to get over it by saying that "evolution is never perfect" and that "exceptions occur" is not "facing the music." Now this story of the degeneration of domestic animals and plants is an obsession in India. I have had occasion to test it in the case of the latter, and satisfied myself that it was due to mongrelising; and, as to Clayton's beans, I completely exploded a similar case in Arabis some years ago in these pages. My own conclusion is that variation is inherent and spontaneous in the germ-plasm; and the "germinal power of resisting enforced change" is an undoubted fact which manifests itself in "specific stability."

The varying germ-plasm inherits and transmits variations. Thus we are led to the thorny question of recapitulation. Sedgwick agrees that it is "a deduction from the theory of evolution," but that it "is still without satisfactory proof." On the other hand, in the same volume, W. B. Scott finds that in brachiopods, "in the more advanced genera, the developmental stages clearly indicate the ancestral genera of the series." The botanist is constantly running up against recapitulative structures. When he finds a trace of a prothallus in a flowering plant and a spermatozoid in the pollen-tube of Salisburia, it is difficult to avoid the conclusion of Bower that land-plants had aquatic ancestors. We must, however, agree with Prof. Sollas that "nature no doubt is a strict adherent to logic, but she betrays a singular want of method in recording the steps of her argument."

Dr. Archdall Reid thinks, and no doubt rightly, that "the main reason against a full acceptance of the Darwinian doctrine" is "the retrogression of useless parts and organs." His solution of this difficult problem is one of the most novel and interesting things in his book, and will probably be subjected to most criticism. Thirty-two yearlings, costing 51,520 guineas, only produced two winners. From this and similar cases he draws the inference that retrogression preponderates over progression. He accounts for it by supposing that there has been a selection of germ-plasms which "tended on the whole to vary retrogressively." But retrogression in turn "is checked only by selection." The difficulty at once arises to reconcile this view with the biometric result which he admits, that "variation tends to occur about equally about the specific mean." Incidentally it may be noted that he identifies retrogression with reversion.

The various solutions of the problem which have been attempted are discussed. There is a risk that the terminology used may cover a *petitio principii*. Given

an organism, how is it to be adapted to a different environment? The adjustment may be effected by further complication or by simplification. It may be noted that in regard to the latter there is a close parallel in the evolution of machinery. Whole trains of mechanism are continually being swept away with an increase of efficiency. Compare, for example, a turbine with a marine engine. Here structural retrogression has made for functional progression. We owe it to Lankester for pointing out that "degeneration" is really simplification leading to closer adaptation. Progress in biology is not ethical, but position in the phyletic scale. The last of the Plantagenets is said to have kept a turnpike; but he may have been not the less authentic.

The instability of prize-bred domesticated races requires careful scrutiny. The late Duke of Devonshire pointed out to Lankester that racehorses are bred for speed and not for "points." The conclusion that I draw from Sir Walter Gilbey's facts is that breeders have not yet succeeded in fixing this particular quality. But short-horns, which are bred for points, have reached a high degree of stability; if they had not no one would give a thousand guineas for a bull. The purchase of a possible racehorse is confessedly a gamble. For my own part, I am content with Lankester's view that nature "with remorseless thoroughness" can throw overboard hereditary tendencies, if it is advantageous to do so; and this is really the same thing as Dr. Archdall Reid's selection of retrogressive germ-plasms, except that he throws on natural selection the burden of defeating its own aim.

Apart from speculation, we have in Galton's law of regression to mediocrity an empirical result which is perfectly general inasmuch as it deals impartially with excess and defect. It produces "a sensible stability of type and variation from generation to generation." It has always appeared to me the most important positive addition to the Darwinian theory, and it has seemed possible that it would open the door to a mechanical explanation of retrogression, or, as I prefer to say, of simplification; and this is apparently in Archdall Reid's mind, as he remarks that "regression is but the first phase of retrogression," though he has not followed it out further. Regression is independent, apparently, of natural selection, while retrogression is not.

This leads to another point which is often overlooked. The mere "maintenance of a structure" is dependent on the continued action of natural selection. As Poulton insists, it is by its operation that "all functional parts of an organism are kept up to a high standard." It may be a private heresy of my own, but I can attach no more meaning to the "cessation" and "reversal" of selection than if those terms were applied to gravitation.

The chapter on Mendel's laws is altogether admirable. It is probably the most luminous account of them which has been published. "There can be no doubt of the actual occurrence of the Mendelian phenomena. We must endeavour, therefore, to estimate the part played by them in nature." Now where species or stable varieties are crossed we get simple blending, as in the Mulatto. "Mendelian reproduction is one of the rarest things in nature." "Mendelian traits . . . are common only when artificial varieties . . . are crossed by man." It would be impossible with any justice to attempt to summarise the argument. The majority of Mendelian traits "are concerned with reproduction." The illuminating conclusion, in which, however, the author finds himself anticipated by T. H. Morgan, is reached that they are analogous to sexual characters which are alternative, *i.e.* are latent or patent in the opposite sex. If this explanation holds good, and it has the obvious merit of including phenomena not obviously connected at first sight, it effectually disposes of "segregation"; and "unit-characters" necessarily follow. But their existence had already become precarious, for Prof. Karl Pearson kindly informs me that he has entirely failed to discover any which, to put it briefly, can be described as having unitary properties. It is pointed out that the inheritance of mutations is alternative, and the inference is drawn that characters which blend in crossing cannot have arisen as mutations.

Lastly, we come to the "Function of Sex." This is found to be an adaptation "to blend parental characters." Further, it is concluded that "blending, with its swamping effects . . . eliminates useless characters and variations." This at once explains retrogression, and at bottom on this head there is probably not much difference between Lankester and the author. Mutations are alternative and Mendelian; fluctuations are blended; whence Galton's law of regression and stability at once follows. "The average experience of the whole race . . . becomes the determining factor in evolution."

Two incidental points deserve notice. Parthenogenesis "occurs as a rule amongst simple forms." But it is found to occur much more frequently than was supposed amongst flowering plants; the dandelion is an example. Still, it may be presumed that sexual reproduction and cross-fertilisation occasionally occur. Fertility, both on biometric and general grounds, is thought to be a transmissible adaptation. Karl Pearson has, however, arrived at the important conclusion that there is "little or no demonstrable inheritance of fertility." Further, he is "forced to the conclusion that the smallness of the hereditary factor in fertility is an essential feature of Darwinian evolution." It is interesting to note that in this case deductive reasoning has led to diametrically opposite conclusions.

This disposes of the first part of the book. I do not know that I have come across anything more suggestive on the subject since the "Origin" itself. It may be added that Prof. Turner has thrown the main argument into a quasi-mathematical shape in the appendix. The latter and larger portion of the book is difficult to review in any reasonable space. It is a striking commentary on the contention of de Vries that organic evolution has nothing to say on social problems. It ranges over a wide field, including even a short system of philosophy, and will probably be found the more interesting because the least technical, and might well have been published separately.

Disease and immunity are admirably discussed. Races become tolerant through selection working on germinal variation. Protoplasm learns to neutralise

toxins. Twenty years ago I ventured with bated breath to hint the possibility of its education. The result is that the microbe and not the sword is the ultimate "empire-builder"; and subject-races will either absorb or expel their conquerers. The argument is extended to alcohol and narcotics. All races who win their freedom from vicious indulgence must first be slaves to it. Insusceptibility to its charm, though not precisely parallel to disease-immunity, is, like it, a product of germinal variation. Meanwhile, selection slowly eliminates those who do not possess it. If it is true that the English are the most drunken of existing races, and that "about one death in seven" amongst them is due to alcohol, it has its work cut out for it. Still, it is at work; and any attempt to interfere with it by the total suppression of alcohol would simply result in the production of a more susceptible race.

Fortunately, though susceptibility is germinal, indulgence is an acquired habit. It follows that the children of drunkards will not necessarily follow in their parents' steps, and Karl Pearson confirms this from biometric data. The same reasoning applies to slum-dwellers. Here also the injury is somatic and not germinal, and would disappear if the conditions were improved; it is not transmitted, but reproduced in the offspring, which the experience of Dr. Barnardo's Homes shows is still capable of healthy development. Slums are continually recruited from outside; it is probable, therefore, that little, if any, germinal mischief has been produced. But it can be shown on Dr. Archdall Reid's own principles that, given time, an adapted and degenerate race would develop, which would be parasitic on the community, and probably prolific.

The chapters on mind I must leave to the psychologist. Lankester is followed in seeing in "the relatively enormous size of the brain in man and the corresponding increase in its activity and capacity," the fundamental distinction between man and other animals. "Educability is nothing more than a power of growing mentally under the stimulus of experience." This is inherited, while the resulting mental acquirements are not. The real test of education is the *quality* of thinking produced. I cannot, however, follow the author in his condemnation of Karl Pearson's Huxley lecture, the conclusions of which I believe to be, not merely perfectly sound, but of the deepest importance. Dr. Archdall Reid tells us that "ability is inborn"; Karl Pearson says it is "bred." I fail to see the distinction. Feeble-mindedness is found to consist in "incapacity to learn" and to be a "reversion to a pre-human mental state." Being germinal, it is inherited, and the community is justified in restraining its marked fertility.

Here I must conclude my review of a very remarkable book; the more remarkable as it is the work of a man somewhat aloof from the scientific world, and written as the recreation of a strenuous professional life. The author invites criticism, and I have not stinted it. He will doubtless get plenty more.

Perhaps Dr. Archdall Reid's more vulnerable point is the superior certitude which he (and Dr. Donkin) claim for deduction over observation and experiment (which is only observation of facts not immediately patent). It is true that when we come across an apparently irreconcilable fact, its improbability depends on the certitude of the law with which it conflicts. It may be due to experimental error in its widest sense; but it may be the germ of a new discovery. Newton laid aside his theory for a time because he could not reconcile it with the moon's motion. But Greenwich did not abandon it when it was found that the path of Halley's comet was not an ellipse. Certitude is built up by accumulated verification. Even mathematics, which are purely deductive, cannot wholly dispense with it. It was long thought that the conversion of linear into circular motion was impossible until Peaucellier effected it. And even so distinguished a mathematician as Sylvester once told me that he had published a number of theorems which, when tested arithmetically, proved to be untrue. Experiment cannot always wait on deduction. Röntgen's great discovery was an accident. A discrepancy in the weight of nitrogen revealed argon. It would possibly have been a long time before physicists found out for themselves Brownian motion and osmotic pressure unless botanists had done it for them. Darwin found by experiment that cross-fertilisation was advantageous to plants, and it is difficult to see how the fact could have been arrived at in any other way.

Huxley must have projected a prophetic eye into the future when he wrote:—

"The great danger which besets all men of large speculative faculty, is the temptation to deal with the accepted facts in natural science, as if they were not only correct but exhaustive; as if they might be dealt with deductively, in the same way as propositions in Euclid may be dealt with. In reality every such statement, however true it may be, is true only relatively to the means of observation and the point of view of those who have examined it. So far it may be depended upon. But whether it will bear every speculative conclusion that may be logically deduced from it, is quite another question."

The warning is not unneeded in many directions. It is, I think, particularly needed in regard to Dr. Archdall Reid's impatience with biometry and taxonomy, or rather, I should say, imperfect acquaintance with their aim and methods. He appears to think that biometric method begins and ends with mere enumeration. But such a research as that of Karl Pearson on the distribution of stars in space would show him that it goes a good deal farther. As Karl Pearson tells us, biology "has now developed theories of such complexity, that without the aid of the highest mathematical analysis it is wholly unable to state whether its theories are accurate or not." For my part, when a distinguished mathematician is willing to devote his splendid gifts to the task, my attitude is not querulous, but one of profound gratitude.

And taxonomy is even less a ground for impatience. For, as Linnæus saw, its real aim is to embrace all organisms in a natural classification. The principle of descent is implicit in this, and it was therefore towards it that all taxonomists were unconsciously working. Far from being hostile, it was amongst the systematists—Hooker, Asa Gray, Bentham, Bates, and Wallace—that Darwin found his most ardent champions.

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